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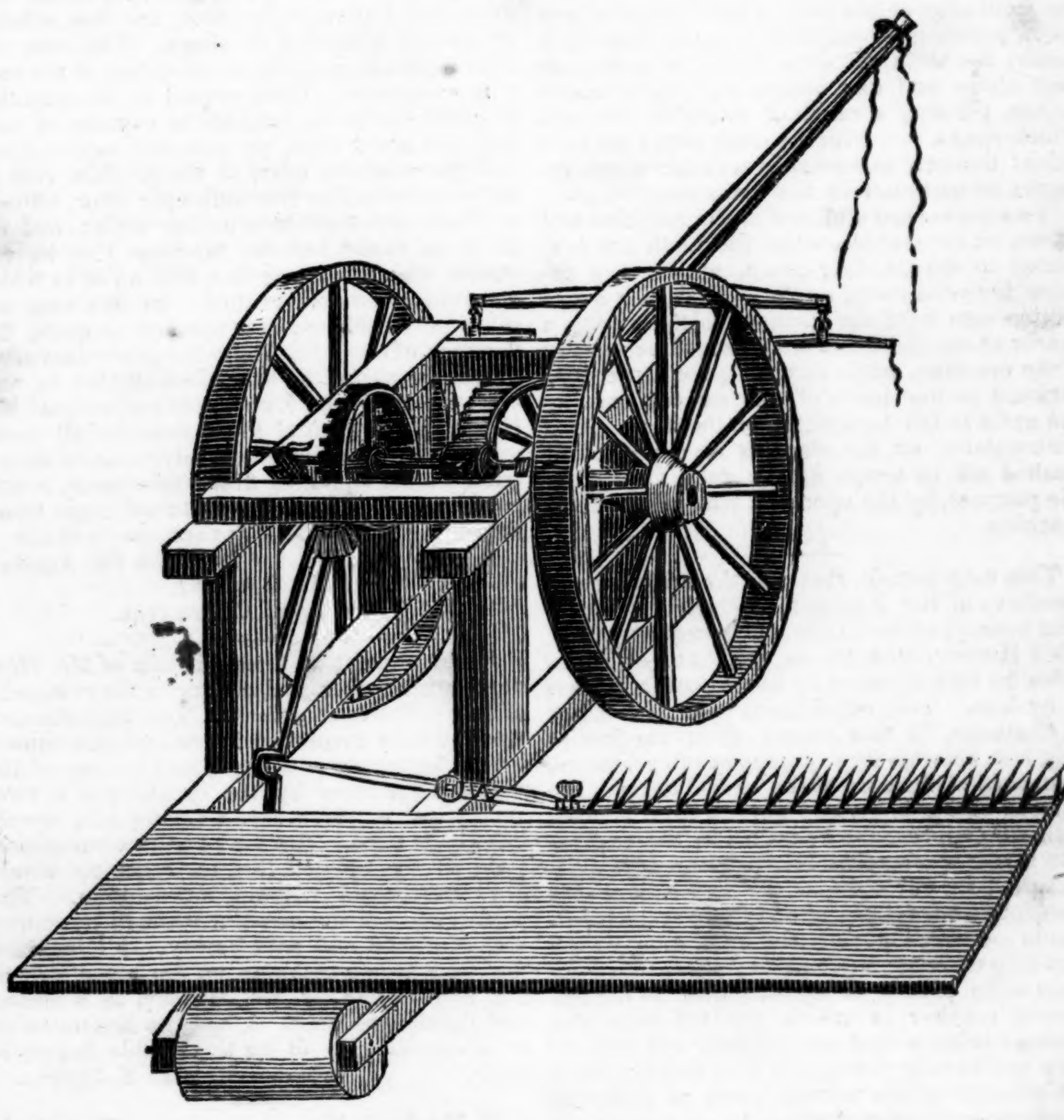
"How unhappy would man be without an interchange of thought! The mind would be as riches coffered; their shining beauties eternally buried and prevented from beneficially circulating among society."—SPECTATOR.

HUSSEY'S GRAIN CUTTER.



Description and Drawing of Hussey's Grain Cutter. Communicated by the INVENTOR.

[We have seen one of Mr. Hussey's grain cutters, manufactured in this city by Messrs. R. Hoe & Co. It is a simple, substantial machine, and from its construction and the perfect manner of cutting a little artificial field of grain, we would add our own recommendation to that of the respectable names in the certificate. The inventor is about taking it into the grain regions of the western part of the state, to exhibit its operation at the next harvest.]

This machine consists of a frame of good oak or ash, sustained by two wheels forward, and one wheel or roller in the rear, and is constructed in the following manner: Two sills are connected by several cross rails; on these sills are fixed four posts; two top rails are framed to the tops of the posts, parallel with the sills, and connected also with cross rails, as seen in the plate. To the forward posts is hung the main axle, with journals running in metal boxes: on this axle the wheels are fixed with square boxes: these wheels sustain the forward part of the machine, and furnish the cutting power. Across the rear ends of the sills is fixed a plank floor of good pine, extending several feet beyond the right wheel. This floor is horizontal, and its distance from the ground will be the length of the stubble. On the front edge of this floor is fixed a row of iron teeth, pointing forward horizontally, forming a comb: the teeth are formed of two parts, one part above and one below, and joined at the points, forming a range of mortices, through which runs a saw with the teeth sharp on both sides: this saw is moved by a crank which receives its motion from the main axle.

Two horses are attached to the machine and driven on the stubble, when the teeth are presented to the standing grain, which they receive between them, as the saw with a quick motion cuts it off, the morticed teeth forming a bearer above and below the saw. The velocity of the machine, while cutting, gives an impulse forward to the butts of the straws, causing the grain to fall backwards on the floor. As it accumulates on the floor, it is deposited or pushed off in heaps with a rake formed for the purpose, by the operator, who rides on the machine.

This may certify, that we, the undersigned, members of the Agricultural Society of Hamilton county, state of Ohio, at the request of Mr. Obed Hussey, attended an exhibition of a machine for cutting grain by horse power, invented by him. The experiment was performed at Carthage, in this county, about the first of July last, before a large company of spectators, composed of farmers of the neighborhood, the citizens of Carthage, and several from Cincinnati, who appeared to be united in the expression that it was a valuable improvement in agriculture. In our opinion the experiment was completely successful, although several impediments occurred during the exhibition by the breaking of some weak parts; these obstructions were plainly to be attributed to the imperfect manner in which the machine was made, it being a first experiment, and experience not having yet taught how to proportion the strength of the several parts to meet the stress which each part might be subject to, on its trial, some pieces being of wood, which should have been of iron; but we have no doubt but all these imperfections can be remedied in a second machine. We were satisfied that the impediments referred to were not to be ascribed to any defect in the principle, for, while the machine was in operation, the performance was complete, until some part broke by the violence to which it was subjected, it having two horses attached to it, and they several times driven on a brisk trot; at this speed the grain was cut as well, or better, than when the horses were driven slow. The machine performed well, both at the rate of two and a half and five miles per hour; and although the horses were several times urged, they were not driven so fast at any time as to determine at what speed the machine might be moved, and do good work. The wheat was found to be cut much cleaner, and to be left in better order for binding, than when cut by the cradle. The saw which cuts the grain was made without a temper for cutting, consequently would not continue sharp long at a time; but no difference was perceived in the execution, the grain being cut equally clean, and fast, whether the saw was dull or sharp. This was attributed to the peculiar construction of the cutting apparatus. With regard to the quantity of grain which the machine is capable of cutting in a given time, we can only say, that we saw the machine move at the medium rate of three and a half or four miles per hour, cutting a swarth five feet three inches wide; and we have no doubt but the machine may be extended with advantage to a half a rod in width on ordinary smooth ground. In this case the machine would pass over one acre in going the distance of one mile. From the general satisfaction expressed at the exhibition alluded to, and our own impressions, we would recommend Mr. Hussey's grain cutter to the notice of all grain growers, being satisfied ourselves, that if future trials should equal the first experiment, it will be a valuable improvement to all large farmers.

D. C. Wallace, Sec'y of the
Hamilton Co. Ag. So'y
J. D. Garrand,
Calvin Carpenter.

I was present at the exhibition of Mr. Hussey's grain cutter, and concur in the statement of D. Wallace and others. The impediments referred to by them were in one instance caused by the loosening of a cog wheel by loss of the wedges, the other by the breaking of a two-inch wood screw, where a strong bolt should have been used. But for these two casualties, I am of the opinion that the machine would have performed without interruption. The performance of the machine while in operation was complete and satisfactory. I have since that time seen a machine on the same principle, constructed by Mr. Hussey, in a strong and durable manner. I have no hesitation in recommending it to be a valuable improvement.

T. B. Coffin.

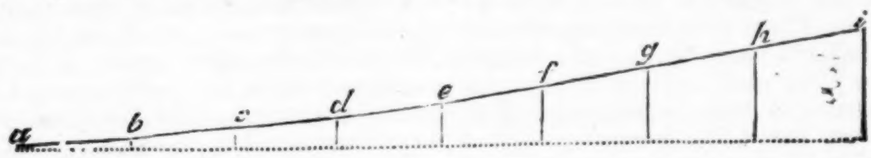
H. Huxley & Co., 81 Barclay st. New-York, are agents for selling the above machine.

The Undulating Railway. [From the London Mechanics' Magazine.]

Sir,—To prevent the possibility of any further misunderstanding in reference to the experiments recently made on the Liverpool and

Manchester railway, I have this morning called upon Mr. Booth, and have compared with his statement thereof the following particulars of the rise of the Sutton inclined plane, from its base to a higher point of elevation than any which was attained in our experiments. To render this explanation more clear, on which reliance may be placed, I have accompanied it by a diagram, wherein is denoted the points from which the ascents and descents, during the experiments, were measured.

I am extremely sorry that these particulars have not before been published, as the want of them has evidently produced inconvenience to some of your correspondents, which, had it occurred to me, I ought not to have permitted.



From *a* to *b*	the distance is	88 yards	—rise 1 in 754	—total elevation	.116 yards
b to *c*	"	88 "	—rise 1 in 122	— "	.721 "
c to *d*	"	88 "	—rise 1 in 105	— "	.838 "
d to *e*	"	88 "	—rise 1 in 97	— "	.907 "
e to *f*	"	88 "	—rise 1 in 94	— "	.936 "
f to *g*	"	88 "	—rise 1 in 92	— "	.956 "
g to *h*	"	88 "	—rise 1 in 89	— "	.988 "
h to *i*	"	88 "	—rise 1 in 89	— "	.988 "
		704 yards		Total rise,	6.450 yards.

The ascent of the Sutton inclined plane may be said (see diagram) to commence at the point *a*; although for a distance of 2½ miles before arriving at that point there is a trifling ascent of about 1 in 2,640—viz. from the Sankey viaduct to the foot of the plane.

Now, the series of experiments first published (see No. 531), and which were tried with the Rocket engine, were made from what was considered the foot of the plane, and which in the diagram is marked letter *b*.

The results of those experiments evidently proved to me that the inclination of that part of *the plane* on which they were tried was not, as generally supposed, 1 in 96. I perfectly recollect Mr. Sanderson alluding to some supposed error, though why the allusion was "*not palatable to me*" is a perfect mystery. It was not, however, until after I sent you the particulars of the experiments, and my return to Douglas, that I had an opportunity of making such calculations as led me to call a second time on Mr. Booth, viz. *before* the trial of the second series of experiments, and explain to him the discrepancy which appeared. For instance, see page 21, experiment 6:—The Rocket engine and a load of 35 tons ascended by *momentum* 134 yards, the velocity at the foot of the plane being 10 miles per hour, which ascent was equal to 4.1875 feet perpendicular elevation. Now, 10 miles per hour is 14.672 feet per second, and supposing friction out of the question, a body having gained a momentum of 14.672 feet per second, by gravity, would only ascend 3.36355 feet. Thus it was evident to me that 1 in 96 was not the proper inclination of that part of the plane. Previously, however, to the experiments afterwards detailed, the levels were taken afresh by the Messrs. Dixon,* and in the experiments tried with the Liver and load on the 16th (see No. 534,) the ascents were measured from that part of the plane marked × in the diagram, at which point there is a cottage on the railway. On the Sunday following it was agreed by the engineers present that the place of starting should be again changed, and the experiments with the double load were all made from that part of the plane from which the Rocket had ascended, viz. from *b*. It will naturally occur to your readers—if Mr. Badnall were acquainted with all these particulars, why did he not lay them before the public? The fact is, that I considered the result of the experiments made with the Pluto and Firefly, with the *double* load, such as to render all explanation unnecessary with regard to the previous trials, and especially *as I stated at page* 71, that the inclination upon which those latter experiments were tried was about 1 in 99, which will be found to be the average. Moreover, I did not at the time consider a knowledge of the exact inclination of the plane at all necessary to a clear comprehension of the nature and results of the experiments, inasmuch as *all I wished to prove was*, that *whether velocity* was generated by one or more engines at the foot of ascent, by which velocity (either with or without the continued assistance of one engine) a given elevation was surmounted, *a greater velocity could be generated by descending the same distance*, evidently proving, beyond all rational doubt, the correctness and value of the principle.

The discrepancy alluded to by "Kinclaven" will be explained, I trust, satisfactorily to him, by referring to the statement of the experiment to which he alludes. In that instance the word *momentum* is not introduced; on the contrary, the whole power of the Liver engine was employed throughout the *whole ascent.* Had this not been the case, there evidently must have been some great error. I need not say that I shall be most happy, not only to give

* I had also an opportunity of comparing, this morning, my notes on the rise of the inclined plane with Mr. Dixon's, sen., from whom I had originally been favored with them.

every further information in my power, but if any of my opponents will propose any further practical test, upon the result of which they will cast the merits of the question, it shall be, if possible, immediately and most impartially tried.

As a proof of the impartiality with which I have recorded the experiments already tried, I refer to all the engineers present, whether the *steam* of the Pluto (see last experiment) was not shut off 155 yards before she arrived at the starting post, which made a very considerable difference in the rise by momentum. Seeing, however, that I had *proved* enough, I neither complained at the time, nor have I hitherto published my complaint. The error arose from the two conductors of the engines shutting off the steam of both engines when the flag dropped for the first engine to shut off on passing the mark, letter *b*.

May I again ask if Mr. Cheverton and "S. Y." will be satisfied that there is an advantage if a given locomotive engine will move, at a given velocity, *double* the load from summit to summit which she is capable of moving on the level at the same velocity? If so, will they, if not satisfied with the impartial judgment of our northern engineers, attend on an appointed day, of which they shall have due notice, and witness the experiments themselves? If they refuse to attend, and if they disbelieve the results of the experiments already tried, it is needless to make a single further comment on their opposition. On the other hand, if they do attend, and if they do witness a decided proof that a load, which *will not move on a level*, will move from summit to summit of an undulation at a *great velocity*, what becomes of the "ASSUREDLY NOT" of the *Champion*—and why is it necessary that "S. Y." should give such friendly advice to Mr. Ham, and to the subscribers of the *great western railway?* I am, however, happy in believing, that a full and impartial trial of the undulating principle will *soon* be made on rather an extensive scale; and I hope "S.Y." will state his *practical objections*, and that the *Champion's rod* may be most freely *exercised* before such *trial takes place*. As to the sickness which these gentlemen complain of, I am sorry I can administer no better restorative than my regret.

I am, sir, with great respect,

RICHARD BADNALL.

Manchester, November 28, 1833.

P. S.—I observe that "S. Y." makes some allusion to "*The Editor of the Manchester Guardian.*" Probably he is not aware that Mr. Garnett, the editor, is an opponent of mine, and *one* for whose mechanical attainments I have a very high opinion.

The Undulating Railway—Resistance from Friction—Resistance of the Atmosphere—Mr. Badnall in reply to S. Y., Junius Redivivus, and Mr. Cheverton. [From the London Mechanics' Magazine.]

SIR,—Seven months have now elapsed since the undulating railway was first introduced, as a subject of discussion, in your Magazine. During that period I have done my utmost, by fair and conscientious argument, to support the cause which I undertook to defend; and the gratification which I now feel in having witnessed your honorable and candid confession of a changed opinion, and in finding myself supported by several of your most able correspondents, far more than compensates for the disappointment which the opposition of "S. Y." would naturally excite, even should it be continued after the publication of this letter, and after the important facts determined by the experiments. I say *disappointment*, because, if still unconvinced, he will, I fear, ever remain inconvincible; and, judging from the occasional piquancy and asperity of his remarks, he is not likely to be more fairly defeated, without losing, in some measure, that evenness of temper which I should be sorry to disturb. If I do him injustice, I apologise for it; but I feel that the time is now arrived when (practical experiments having decided the merits of the question) I have no longer occasion to defend myself by parrying the verbal attacks of my opponents. On the contrary, I waive all further hypothetical discussion on this subject, unless such discussion refer to the result of my late or future experiments. In coming to this conclusion I am not considering my own convenience, but I think your readers in general will agree with me, and with your friend, *Professor Crackwell*, that unless we draw in our horns, the undulating controversy will not only become sickening, but, judging from Mr. Cheverton's last letter, somewhat disgusting. "*Nec lusisse pudet, sed non incidere ludum,*" said Horace, and I quite agree with him.

You, Mr. Editor, or an impartial jury of your readers, must judge whether I am led to this train of thinking through *fear* of my opponents, or whether I am not justified in claiming the victory I have contended for. Those gentlemen who have advocated my side of the question—*Saxula, Mr. Ham, Mr. Sanderson, Kinclaven, Mentor*, and *Mr. Trebor Valentine*,—have each and all supported my position by convincing diagrams, appropriate comparisons, or disproved experiments; whereas neither "S. Y." nor Mr. Cheverton have thought proper to substantiate their reasoning by a single particle of corroborative evidence. That both are clever men, I do not for one moment question; but a clever man occasionally errs; and never is he more likely to do so than when inflated with that unhappy quantity of combustible matter,—vulgar abuse, self-sufficiency, and extreme vanity,—which have been so conspicuously displayed in the disjointed lectures which Mr. Cheverton has directed to me on this subject. For those lectures I am indebted to him, especially for the last, which I shall presently take into consideration, and which, I trust, will be headed in your title-page, "THE PROFOUND IGNORANCE OF MR. BADNALL DEMONSTRATED BY THE SUPREME SENSE OF MR. CHEVERTON"!!

My present object is to reply to all unan-

swered objections which have been raised by my opponents up to this time. In doing this, I may probably introduce some opinions which may appear open to further discussion; but as I fully concur in the sentiments expressed by ϕ. μ. (No. 532), as to the frequently injurious effects of a too protracted controversy, I shall feel it an act of duty to your readers to be a silent observer of any attacks upon them. I place them on record as my deliberate and conclusive opinions; and having done so, I turn from *theory* to *practice*, and now present myself to your readers as the defender of the undulating principle in a far more important point of view—I mean in defence of its *complete practicability*.

In thus a second time throwing the glove, allow me to prognosticate what will be the result of another year's experience. Within that period, engineers and mathematicians will have an opportunity of making up their minds upon the subject, and from the expiration of that time *we shall never have another level railway* (whereon locomotive steam force is intended to be employed) *laid down in Great Britain*. The Liverpool and Manchester railway, though it will ever maintain the character of being one of the most important examples of British spirit, British perseverance, and British ingenuity, will, in the eye of posterity, have one dark spot upon its fame—it will be compared to the massive and expensive aqueducts of the ancients. Our forefathers knew not that water would find its own level —and, while we praise their structures, we cannot help wondering and smiling at their ignorance. Thus, however, will posterity smile at us, exclaiming, "*Could you have believed it! They expended, in about thirty-one miles, hundreds of thousands of pounds to make a railroad level*, through their disbelief *that all bodies descending on a curvilineal arc will rise again to their own level, minus friction!*"

I now turn to "S. Y."—a few words afterwards to Junius Redivivus; and then, in perfect good humor, to Mr. Champion Cheverton.

A desire to remove, if possible, every opposition founded on mathematical reasoning, which has been urged against the undulating theory, induces me to return to "S. Y.'s" first communication. Before doing so, however, I must at once contend against the liberty which he takes in stating that I have betrayed ungentlemanlike conduct by my observation, "that I should have felt hurt that any other correspondent than himself had doubted my proficiency in common arithmetic." The "indignation" of "S. Y." cannot possibly justify such an observation.

In "S. Y.'s" letter, page 181, there is an error in print, afterwards corrected, which rendered his first formula "incomprehensible." I allude to the omission of the decimal dot before the figure 8. In the succeeding column, however, I find this misprint did not occur; I therefore ought to have understood his object better than I did. But allowing that I had fully comprehended it, and that such misprint had not occurred, I observed that the whole formula was founded on false data, and that the position which he took was altogether untenable. I refer now to the saving of friction "*abstractedly*," without allusion to the difference in velocity occasioned by the action of gravity, to which latter point he also frequently alluded, when he denied that the speed could be greater on a curve than on a level line. With regard, then, to the real difference of friction on the two roads, he gives the following proposition, which I have thought it better to describe by diagram:

Fig. 1.

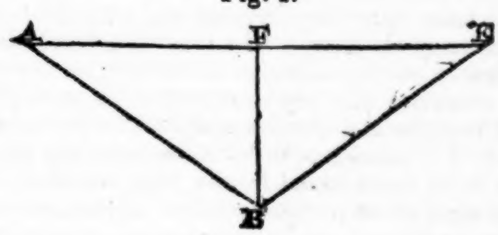
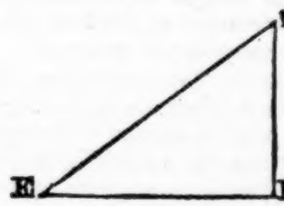


Fig. 2.



"If A E (fig. 1) be equal to 16, and the depth F B equal to 6, the length of each inclined plane will be equal to 10, and the pressure against the plane; and therefore the friction, according to Mr. Badnall, will be equal to ·8 of the friction on the level."

Now "S. Y." must have misunderstood my diagram, *p. 93, to which he refers, and which was, I think, clearly elucidated. If he *did understand it*, from whom did he draw his conclusion, that I imagined the friction or pressure on the inclined plane E B, whose perpendicular elevation is equal to F B, equal to ·8 of the friction on the level?

Let him suppose, then, (reversing the above diagram, as fig. 2,) the plane E B raised upon the base E F, at an elevation of F B. It cannot be doubted (as proved by the parallelograms described in my diagram, page 93,) that if the base line E F represent the pressure (or friction) of the whole weight resting on the plane E B, F B will represent the force of gravity down the plane; or, in other words, as the length of the line F B is to the length of the line E F, so is the pressure or friction taken *off* the inclined plane E B to the pressure or friction *left on the inclined plane*; or, to be more explicit, if a body be supposed to weigh 10 tons, and to be placed on the hori-

* On referring to this diagram I find the length of the level line E A=22, the length of each inclined plane=5¼ and the elevation=2·5; if, therefore, we deduct 2·5 from the length of F A, we shall find the reduction of friction of pressure nearly one-fifth.

zontal line E F, no one can dispute that, the line of pressure being vertical, the *whole weight of the mass* must necessarily press upon the rail. If, then, E F were exactly equal to F B, and the weight were removed to the inclined plane E B, the pressure would be reduced *one half;* and thus, in the above diagram, E F being equal to 8, and F B equal to 6, and supposing any weight resting on E B to be divided into 14 parts, $\frac{8}{14}$ths of the whole weight would be resting on the rail, and $\frac{6}{14}$ would be taken *off* the rail.

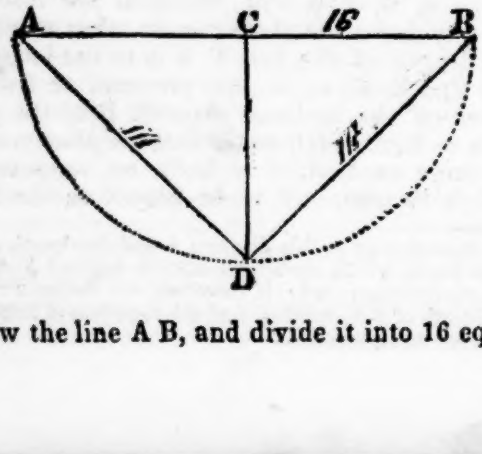
By this explanation it will be evident that "S. Y.'s" second formula, page 242, is, like to his first, established on wrong data, *for he never takes into consideration the perpendicular elevation of the plane;* and it is this which has evidently misled him, or otherwise he would not consider his argument to hold good for "all lengths and *elevations* of inclined planes."

"S. Y." considers in both formulæ the pressure to be determined by the *base*, divided by the *length of the inclined plane:* he consequently draws in each case an erroneous conclusion, for there can be no doubt whatever that, *as the perpendicular height of the inclined plane is increased, the pressure or friction of any carriage moving on that plane is reduced.*

Referring to the preceding diagrams, nothing can be so easy as to determine the *exact* proportion which subsists between the pressure or friction on an inclined plane, and the pressure or friction on a horizontal plane, provided the angle of elevation be given. In the case before us we have the angle F E B. Now, let P be the pressure on the base, or horizontal line F E, and let p be the quantity taken off that pressure, owing to the inclination of the plane, and let a be the angle of inclination: we then have *in all cases*—

$$p : P :: FB : FE$$
$$\text{but } FB : FE :: \text{tangent } a : 1$$
$$\text{therefore } p : P :: \text{tangent } a : 1$$
$$\text{and, consequently, } p : P \text{ tangent } a.$$

If, then, the amount of friction or pressure on an inclined plane (speaking abstractedly of friction or pressure) be reduced in proportion to the angle of inclination, it must, I should hope, be evident to "S. Y." that his position is wrong. He, no doubt, will allow that the pressure at an angle of 45° is reduced *one half.* To make myself, therefore, perfectly clear, I will take this angle to prove his formulæ *incorrect*, and the undulating theory, *in regard to friction, perfectly correct:*



Draw the line A B, and divide it into 16 equal parts. From the centre C describe the semicircle A D B. Draw the line C D perpendicular to A B, and from the points A and B draw the straight lines A D, B D.

Now, as before observed, because the perpendicular line C D is equal to the line B C, any weight descending on the line B D will press with exactly half the force with which it would have pressed on the level line A B, the angle C B D being an angle of 45°. Divide, then, the lines B D, A D, into an equal number of parts, each part being equal to $\frac{1}{16}$th of the horizontal line A B=$22\frac{2}{5}$. Next suppose a body, weighing 10 tons, to press upon every described part of the line A B, in passing from A to B: we then have $16\times10=160$; but if 10 tons press upon each part of the horizontal line, *half that weight*, according to the proposition, will only press upon each part of the lines B D, A D.

We have, therefore,

$$22\tfrac{2}{5}\times5=112$$

and, consequently, $160-112=48$ difference in total pressure.

But it may be argued, that if the semicircle A D B were divided into an equal number of like parts, the total number to be passed over on the curve would be 25·142; but this argument will not obtain, it being mathematically true (see Sir Isaac Newton, Parkinson, Hutton, and others,) that the velocities which bodies acquire in falling either down inclined planes, or curvilineal arcs, are precisely alike, viz. as the square roots of their perpendicular heights; and if the acquired velocities are equal, it is self-evident that the resistance opposed to motion down each line is also equal.

Let us now examine to what result "S. Y.'s" formulæ would bring us.

Let B represent B C
L " B D
n pounds equal to the force of traction on a level at any given velocity.

Then the pressure on the line B D (according to "S. Y.") will be to the pressure on the level as $\frac{B}{L}$ is to 1; and therefore the force of traction required in consequence of friction on the inclined plane, will be to the force of traction on the level as $\frac{B}{L}n$ is to n. According, then, to "S. Y.," the entire expenditure of power to move the wheel the horizontal distance on the level will be Bn, and on the inclined plane it will be $\frac{B}{L}n \times L =$ Bn as before. Thus he makes, at an angle of 45°, the pressure or friction on the lines A D and B D equal to the pressure or friction on the horizontal line A B; whereas I make the difference in friction as 7 to 10, or $\frac{3}{10}$ths in favor of the semicircle A D B.

Again, referring to "S. Y.'s" letter (No. 531), wherein he fully explains the bearing of his formulæ, the erroneous view which he has taken of the question is again evident. For, taking 5 ounces (as he suggests) to represent

the force of traction on the level, and applying his observations to the angle of 45°, we shall find that the calculation will not be, as it would arise if his formulæ were correct, viz. $16\times5=80$ on the level, and $22{\cdot}40\times{\cdot}71428571=15{\cdot}999$, or very nearly 16; consequently, $16\times5=80$ on the inclined plane, as before.

But, instead of the force of traction on the semicircle being equal to the force of traction on the horizontal line, we should have it as follows:

$16\times5=80$ on the level,

$22{\cdot}40\times2{\cdot}50=56$ on the curve,

precisely agreeing with the reduction of friction before mentioned, viz. $\frac{3}{10}$ths in favor of the semicircle.

So much for the question of friction, considered abstractedly, and as *commonly* understood; but it must be evident to every man who has perused the particulars of the experiments, that the amount of reduced friction, as in this instance considered, according to the angle of the inclination of the plane, cannot be taken as the precise measurement of power saved, by the adoption of the undulations. On these interesting points I trust that some valuable information may shortly be laid before the public in a treatise on railways,* locomotive engines, &c., which Mr. Robert Stephenson, sen., and myself, are preparing for the press. Previously to that time we shall try various experiments, and I have no doubt, from the plans which we intend adopting, and the precision with which the experiments will be made, that the laws of motion and resistance, under various circumstances and velocities, will be more clearly developed than they have hitherto been. The results which I anticipate lead me to quote a remark of Hooke's in the year 1666: "Gravity, though it seems to be one of the most universal, active principles in the world, and consequently ought to be the most considerable, yet has it had the ill fate to have been always, till of late, esteemed otherwise, even to slighting and neglect."

I have no apology to make to "S. Y." for considering L as a proper symbol for the *length* of the plane, which was ascended with a given velocity, especially as the spaces in most of the experiments varied on every trial. For an error in the last equation, I, however, have to apologise, and I must beg "S. Y." to read L+D for L D—. The word "INVERSE," which he alludes to in his letter (No. 531, p. 23,) was an unintentional omission of mine (see page 222), where, for "*is in proportion*," I evidently intended to say, "*is in inverse proportion*." Any person who reads the sentence will, I hope, give me credit for this.

In reply to the observation of Junius Redivivus, (No. 532, page 38,) let me beg him to place a heavy ball upon a plank, then raise the plank to a vertical position—he will allow that, because the weight falls perpendicularly, there is *no pressure* on the plank. Let him, then, raise the plank on which the same weight rests to an angle of 45°. He will, no doubt, admit that the weight will descend, and that the velocity of descent on the effect of the force of gravity will be in proportion to the diminution of pressure or friction on the plank. Let him next support the weight on the plank (the latter still being inclined at an angle of 45°), by placing his hand under it, or some machine by which he can accurately measure the pressure: he would, I have no doubt, find that precisely *half* the weight was resting *on the plank*, and *half* the weight *upon his hand*, or upon the instrument by which he was measuring the pressure. Let him, then, withdraw his hand, and what becomes of the weight? Half is still remaining on the plank, and the other half is suspended in the atmosphere until it reach the earth which attracts it. Surely, on consideration, Junius Redivivus will acknowledge the truth of this reasoning, and if so, he cannot dispute that the greater the angle of the inclination of the plane the less will be the pressure or friction of any body, either ascending or descending, on such plane.

And now for Mr. *Champion Cheverton!*

The first explanation which I think due from me to your readers, and to which "*The Champion*" principally alludes, is in reference to *the resistance of the air*. I stated, in a former letter, that I thought that *the resistance of the atmosphere* did not (a constant power being employed to urge the body forwards, or, like gravity, downwards,) increase as the squares of the velocity—that the resistance of the air does not act as a greater opposing force (alluding, particularly, to the flight of birds, and to the motion of railway carriages,) at high velocities than at low velocities—that, consequently, the velocity of a train of carriages, supposed to be descending an inclined plane of interminable length, *never could in practice become uniform;* but, on the contrary, that in *theory* the uniform acceleration would not begin to cease until the resistance of the air was equal to the force of descent, which it could not be until the body had attained a velocity equal to that at which air would rush into empty space. I further stated, that it was my opinion "that the resistance of the air, when first overcome by any locomotive force which is constantly and equally *continued*, does not, throughout EQUAL SPACES or DISTANCES, act as an opposing force with greater intensity at high velocities than at low velocities"; but that, on the contrary, it was my opinion that the *total resistance* of atmosphere, throughout a *given distance*, is *less* at high velocities than at *low velocities, from the inclination* which all bodies have to rise from the surface of the earth *when in rapid motion*, and, consequently, from a denser to a lighter atmosphere.

Now, sir, I should have felt not only that an explanation, but that a public apology was due from me, had I published these opinions, without having *very strong reasons* for believing

* The resistance of the atmosphere, and the *cause* of that resistance not increasing as the squares of the velocity, will be particularly elucidated in this treatise by careful experiment.

them to be true. I know they are diametrically opposite to received opinions: *so was the undulating railway;* but time, and careful experiments, will prove whether I am right or wrong. I will now explain my reasons for believing that *I am right.*

In the first place, that there are many doubts existing as to the *true* theory of atmospheric resistance is evident, by the following remark by Hutton: "We conclude (he says) that all the theories of the resistance of the air hitherto given are very erroneous, and the preceding one (alluding to the generally entertained opinion) is only laid down till further experiments on this important subject shall enable us to deduce from them *another* that shall be more consonant to the true phenomena of nature." Surely this admission is a sufficient apology for the humble attempt which I have made, and for the attempt which Mr. R. Stephenson and myself are now making, to investigate this subject.

I must now request the attention of your readers to the following experiments, tried down inclined planes, by Mr. Nicholas Wood, with a view of measuring the friction of railway carriages. (See his work on Railways, 2d edition, pp. 211–213, &c. &c.)

Mr. Wood, in reference to these experiments, thus writes: "Standing on the end of a carriage, and aided by an assistant, at the end of every ten seconds I made a mark upon the plane where the carriage happened to be, and afterwards measured the distance between those marks, which gave the space passed over in each successive period."

Carriage weighing 9,100 lbs.; wheels, 34 inches; axle, 2¼; friction, 44·62 lbs.

Seconds.	Feet.	Real space, the descent not being uniform.	
In 10 the body fell	6·6	6·	feet.
20	26·4	26·4	"
30	59·4	59·8	"
40	105·6	106·2	"
50	165	165	"
60	237·6	242·8	"
70	321·4	326·7	"
80	422·4	424·3	"
90	534·6	525·3	"
100	660	635·5	"

The above experiment was tried at the Kenilworth colliery.

Now, in examining the result of this experiment, if Mr. Wood were correct in his measurement, and upon his correctness I have placed dependence, it is evident that the resistance of the atmosphere did not increase as the squares of the velocity of the moving body, but that, *for some reason or other*, with which reason the public will soon, if I mistake not, be acquainted, it was equable in effect through equal spaces throughout the entire distance of descent.

We know that if a body fall, in vacuo, a given space in the first second of time, it will have fallen four times the space in the two first seconds; that if it fall 16·1 in one second, it will have fallen 64·4 in two seconds; because 16·1 ×4 (4 being the square of the times)=64·4.

Again, if it fall 1608 feet in 10 seconds, it will fall 6433 feet in 20 seconds, or twice the time; because (omitting fractions) 1608×4= 6432.

Now it appears, according to Mr. Wood's *measured* experiments, that in 10 seconds the carriage fell 6·6 feet, and in 20 seconds 26·4.

Now 6·6×4=26·4, which is in exact accordance with the laws of falling bodies.

Again, in 40 seconds, the carriage fell 105·6, and in 80 seconds 422·4.

Now 26·4×4=105·6
and 105·6×4=422·4.

Again, in 30 seconds it fell 59·4, and in 60 seconds 237·6.

Now, 59·4×4=237·6.

Lastly, in 50 seconds it fell 165, and in 100 seconds 660;

and 165×4=660.

Now, had the resistance against the rolling carriage increased as the squares of velocity, the descent *could not have been in accordance with the laws of bodies falling in vacuo.*

I will, however, refer to other experiments, and try the question by another test:

Descent of loaded carriages weighing 9,408 lbs.; wheels, 35 inches diameter; axles, 3 inches.

In 18 seconds the carriage fell	25	feet
28	71·9	"
38	124·6	"
48	205·2	"
58	276·5	"
68	384·7	"
78	506·1	"
88	645·5	"
98	785·3	"
108	939·6	"
118	1081·6	"
128	1266·5	"

Fall, 1 in 104—friction, 41·45 lbs.

Now, in vacuo (taking 16 ft. as the correct measurement in the first second of time), a body in 18 seconds would fall 5210·892 feet, and in 28 seconds 12608·072 feet, and in 38 seconds 23223·852. Now, according to the preceding experiment, the carriage fell 25 feet in 18 seconds; 71·9 in 28 seconds: and 124·6 in 38 seconds: Therefore,

In open atmosphere. In vacuo.

$$71{\cdot}9 \div 25 = 2{\cdot}876, \text{ and } \frac{12608{\cdot}072}{5210{\cdot}892} = 2{\cdot}419.$$

Again, omitting fractions,

In open air. In vacuo.

$$124 \div 71 = 1{\cdot}746, \text{ and } \frac{23224}{12608} = 1{\cdot}842.$$

Again, to make the proof more indisputable (relying upon the measurement of Mr. Wood), we find that, according to his experiments, the carriage descended, omitting fractions, 25 feet in 18 seconds, and 1266 feet in 128 seconds. Now, as before observed, a body would fall, in vacuo, in 18 seconds, about 5211 feet, because 18×18×16·083=to the total space; and in 128 seconds it would fall 263503·872 feet.

Now 1266÷25 (in air)=50·64;
and 263503÷5211 (in vacuo)=50·56.

How very striking, then, is the proportion which the falling body bears in vacuo to the descending body, when opposed to the resist-

ance of the air! So much so, that Mr. Wood must either have imposed upon the public, which I do not and cannot believe, or his experiments, though not intended to elucidate the theory of resistance, are a death-blow to the previously admitted opinions on this subject.

Again, referring to Mr. Wood's experiments (see page 225), we find a perfect regularity in the descending motions; for instance, the carriage was 29·16 seconds in moving 100 feet, and 58·33 in descending 400 feet.

In other instances:

Time in descending 100 feet.	Time in descending 400 feet.
29·10 seconds	58·10
30	60·41
29·16	58·75
31·95	64·35

and all with different loads, varying from 1,120 to 8,960 lbs.

Again, page 226, when the carriage was loaded with 8,960 lbs. it fell 100 feet in 29 seconds, and 400 feet in 58.

Again, in 29 seconds it fell 57·90 feet.
Again, 29·10 " " 58·40 "
Again, 29·74 " " 60·25 "
Again, 31·88 " " 63·75 "
the weights varying as before.

We will next observe whether the proportions were regular. In doing this we find (page 225) that the carriage, with a load of 1,120 lbs. fell 200 feet in 45 seconds, and 300 feet in 55 seconds. Now, in vacuo, a body would *fall* in
45 seconds - - - 32568·075 feet,
and in 55 seconds - - - 48651·075 feet.

Now $300 - \frac{1}{3} = 200$, the fall in 45 seconds on the inclined plane; and $48651 - \frac{1}{3} = 32434$, showing a difference of only 134 in about 32,000.

In another experiment, with 4,480 lbs., the carriage fell 400 feet in 60·41 seconds,
and 500 feet in 67·91 seconds.

Now, in vacuo, a body would fall in
60·41 seconds, 58512·7871523 feet;
and in 67·91 " 74315·8133523 "
therefore, $400+\frac{1}{4}=500$
$58512+\frac{1}{4}=73140$, showing a difference in comparative velocity not worth noticing.

Again, with 1120 lbs., in which instance, owing to the lighter weight, the resistance of the air ought to have been the most felt, we find the body descending,
In 64·35 seconds, 400 feet
72·64 " 500 "
Now $400+\frac{1}{4}=500$, and
$66958+\frac{1}{4}=83247$, showing a difference which is altogether immaterial; for had the distance traversed been 400 and 510 feet, instead of 400 and 500, the proportions *in vacuo* and in open atmosphere would have been precisely alike. Surely, then, these 10 *feet*, considering the variation of friction, by the occasional rubbing of the flanches against the rail, will be regarded as a difference altogether independent of the resistance of air!

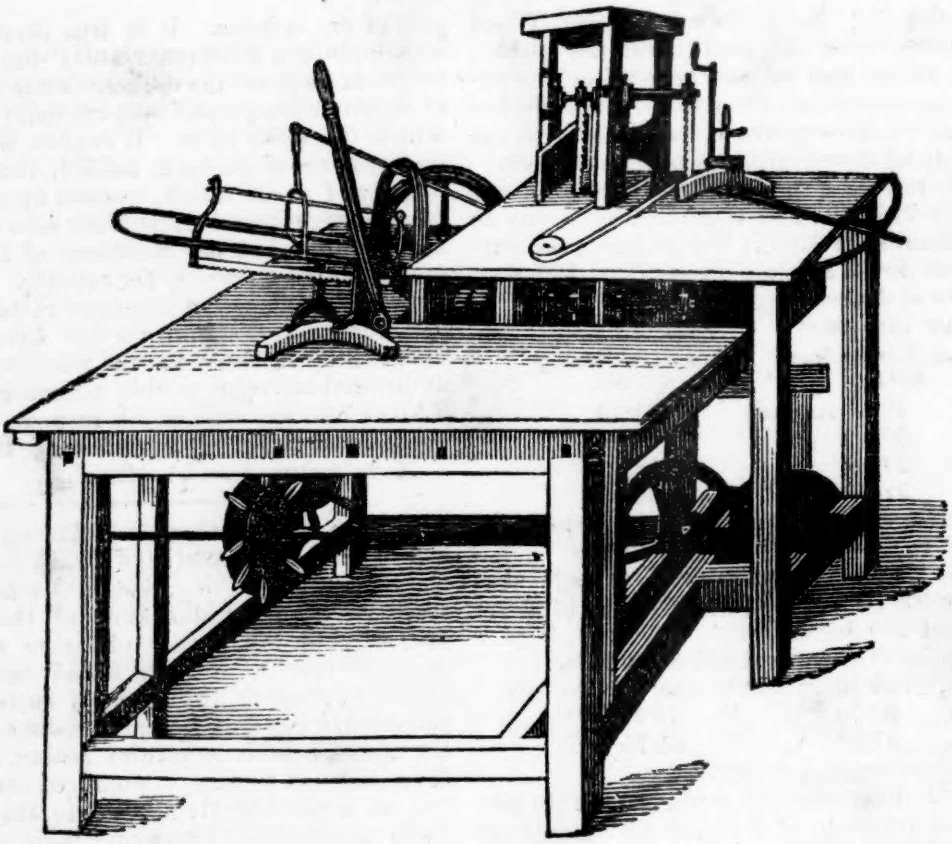
There are many more experiments of Mr. Wood's to which I could have referred in support of my opinion. It is true there are some which show a different result; but the effect might arise from the different state of the rails at different times, and the particular point from which the wind blew. It cannot, however, be doubted, or, if doubted, denied, that the uniformity of acceleration, proved by the experiments herein detailed, *could not have occurred in any instance had the resistance of the air increased as the squares of the velocity.*

I shall, in a further communication, turn to my recent experiments on the Liverpool and Manchester railway, for the purpose of adding additional strength to this argument. Meanwhile I am, sir, with much respect, your very obedient servant, RICHARD BADNALL.

Manchester, Nov. 11, 1833.

CAUSES OF INDIGESTION.—Among the chief causes of indigestion (says Dr. Wilson Philip, in his treatise on this disease,) which act directly on the muscular fibres of the stomach, are narcotic and other offensive substances received into it. I have found that although opium applied to the external surface of the alimentary canal and heart produces no sensible effect on their muscular power, applied to their internal surface, it produces the same effect as when directly applied to the muscular fibres themselves, impairing their power, unless the quantity be extremely minute, and instantly destroying it if the quantity be considerable. It is probable that other offensive substances acting on the stomach—tobacco, distilled spirits, strong peppers, those of an acrid or putrid nature generated in the stomach itself, &c., may also in the same way immediately affect the muscular fibres. It is not uncommon for a fit of indigestion to be induced by taking suddenly considerable quantities of iced fluids. Violent and repeated vomiting also debilitates the muscular fibres of the stomach. But of the causes which immediately affect them, the most frequent and powerful is morbid distention, the most common cause of which is eating too fast; another frequent cause being high seasoning and great variety of food, or such as particularly pleases the palate, by which we are induced to eat after the appetite is satisfied.

FEMALE SUPREMACY.—By all external symptoms, says an amusing writer in this month's Metropolitan, we may apprehend that the reign of women is fast approaching: look at the present aspect of Europe; a Queen of Spain, a Queen of Portugal, a prospective Queen of England. So that we are, at last, to be duly brought under "petticoat government." There is, too, Mrs. Norton conducting a magazine, and Mrs. Cornwall Wilson a weekly publication. Have not women invaded literature and art in all its branches—nay, the most awful arcana of science? There is Mrs. Somerville teaching us the mechanism of the heavens; while Miss Harriet Martineau gives us lessons on political economy.—[London paper.]



HAMILTON'S SAWING AND BORING MACHINE.—This machine is designed for sawing and boring wood or timber, and is claimed by Colonel Hamilton in his specification to be "*an improvement in the mode of sawing felloes of wheels, circular and curved segments*, mitre joints, tenons, and also boring of felloes and hubs of wheels;" and generally for sawing circular, curved, and plain surfaces.

The machine is propelled by a two horse power steam engine. Animal or water power may be applied for the same purpose. The particular form required is sawed out of the timber with perfect accuracy and great expedition, by means of one or more thin narrow saws moving up and down. There is also belonging to this machine a horizontal saw for cutting segments of circles their proper lengths, and with proper inclinations for joints, tenons, &c. &c. Hubs of carriages are bored with perfect precision. All these operations are effected by the changing position of the material, accommodating itself as it comes in contact with the saw or auger, so as to receive the exact form, inclination, &c. required. Every thing is done, without marking or laying out, with mathematical accuracy by means of scales, which are distinctly laid down on the machine.

The machinery which guides and steadies the material in its movements may be readily varied, so as to form segments of wheels of greater or less dimensions; and the boring may also be more or less inclined. The scale indicates the exact position which the part of the machine that guides the material required to form a wheel,—for instance, of greater or less circumference. Slats and legs of chairs may be made of various lengths, and thicknesses, and shapes, as fashion or utility may dictate.

This machine affords a happy specimen of labor saving, and may be advantageously applied to a variety of useful purposes. It occupies but little space, only a part of a small room. No skill is required in using it. A mere laborer, or a boy, can learn in a few hours to use the machine, and to produce the article as perfect as the most skilful machinist. Like many other labor saving machines, it performs that part of the labor which the accuracy and strength of the human hand are incompetent rapidly, and with precision, to perform; it, in fact, does the work which is the most difficult and toilsome to the laboring manufacturer.

The expedition with which materials of small value, and with very little waste, are converted into articles of comparatively much greater value is entitled to particular notice. Chair backs sawed from our native curled maple are worth from *eight to twelve and a half dollars* per hundred.

By the aid of this machine, which costs only about *three hundred dollars*, a common

laborer may do the work of twenty or thirty mechanics. The merit claimed by Colonel Hamilton consists chiefly in the facility and accuracy with which the material is adapted to the saw, so as expeditiously and uniformly to produce the exact form which is wanted.

A Compendium of Civil Architecture, arranged in Questions and Answers, with Notes, embracing History, the Classics, and the Early Arts, &c. By Robert Brindley, Architect, Surveyor, and Engineer. [Continued from page 181.]

NORMAN STYLE.

Q. What is to be said of the Norman architecture?

A. The rich Norman Barons, after the conquest, began to build magnificent castles and churches, introducing architecture on an extended scale, and in a style peculiar to themselves.

Q. How long did the Norman style prevail?

A. To the end of Henry VI. 1189. It seems rather to have commenced before the conquest, but there are no attesting remains.

Q. What is the criterion in determining Norman architecture?

A. This style, as well as the three subsequent ones, may be divided into the following sections: namely, doors, windows, arches, piers, buttresses, tablets, niches, ornamental carvings, and steeples.

Q. How is the Norman door distinguished?

A. The arch is semi-circular; on which are a vast number of bands and mouldings, which increase the depth, and give great richness. Shafts are often used, though sometimes one door is decorated and the other not. An impost moulding surmounts the shaft before the arch moulding springs. These mouldings are much ornamented. The wave and zigzag ornaments are universal, as is also the beak-headed moulding.

Q. How far does the external moulding of the arch extend?

A. No lower than the spring of the arch. In resemblance it is a dripstone, though not projecting as such.

Q. Of what shape is the door?

A. The door is often square, and deeply recessed; the internal part of the arch being ornamented. Iffley church, near Oxford, affords very fine specimens of Norman doors, having three, and all different. Durham, Rochester, Worcester, and Lincoln cathedrals, have also very fine doors. The ornaments are all external; the insides quite flush.

Q. Of what description are the windows?

A. Very diminutive. In large buildings shafts are frequently used. There are no mullions. The arch is semi-circular, and the aperture splays both internally and externally. The bottom is horizontal. There is no attempt to feathering either doors or windows. Kirstall Abbey has all its work exteriorly round arches, though the nave has pointed. There are a few horse-shoe arches and double arches introduced, some with plain faces, but mostly ornamented with the zigzag.

Q. What of the Norman piers?

A. They are of four descriptions: 1st, the *round massive column pier*, having sometimes a round, and sometimes a square capital—plain, or with channels in various forms. It is found from two to six or seven diameters high. The square headed capital is generally the tallest. 2d, a *multangular pier*, less massive, used octagonal and common, with an arch more or less pointed. 3d, *a common pier*, with shafts; this has a square capital, but much ornamented with foliage. The shafts are mostly set in square recesses. 4th, a *plain pier*, with plain round arches in two or three divisions. In some cases shafts are divided by bands.

Q. Describe the Norman buttresses?

A. They have plain broad faces with small projections running up to the cornice tablet, sometimes finishing with a slope, and, in a few instances, composed of several shafts.

Q. What of the Norman tablets?

A. The tablet usually called the cornice is frequently only a plain face of a parapet, projecting the same as the buttresses; but under is placed a row of blocks, either plain or carved in grotesque heads; a plain string is also used as a cornice.

Q. What is the next tablet?

A. This is the dripstone, or outer moulding of windows and doors. It is sometimes undistinguished; oftener a plain round or square string, continued horizontally from one window to another round the buttresses.

Q. What of the other tablets under windows?

A. They are generally plain slopes above and below a flat string. In the interior, and sometimes the exterior, these are much carved in devices.

Q. What are the Norman niches?

A. They are a series of small arches, with round or intersecting arches, sometimes without, but oftener with shafts. Some have their mouldings ornamented.

Q. Name the Norman ornaments.

A. The first and most frequent is the zigzag or cheveron moulding, used in great profusion; and the second is the beak-head on doors, consisting of a hollow, and large round.

Q. What is the Norman steeple?

A. A massive tower, seldom rising more than a square above the roof. It is sometimes plain, but mostly ornamented by plain or intersecting arches, and has the flat buttress. That of St. Albans runs into a round turret, at each corner of the upper stage.

Q. Are the Norman steeples crowned with pinnacles?

A. No; but there are some turrets crowned with large pinnacles, which may be Norman. Such is at Cleve, in Gloucestershire; and such was one of the towers at the side of the west front of Rochester cathedral, which has lately undergone a complete change in repairs.

EARLY ENGLISH STYLE.

Note—The early English and subsequent styles are frequently confounded with the Gothic, by the appellation of *modern Gothic*, and certainly very improperly. The classification of styles handed down to us decidedly demonstrate the error. This appellation was first bestowed by the Italians on some species of ecclesiastical architecture, as an epithet of obloquy, "*La manera Gotica*," signifying its supposed barbarous derivation from the Grecian or Roman models—not implying its procedure from the Goths; who possessed no *national* style of architecture, and who, when in Italy, profited by Italian artists.

The most correct term would be *Saracenic*. Wren was of this opinion, attributing its refinement to the Christians, after the fall of the Grecian empire. Salisbury Cathedral Church, finished in 1258, is entirely Saracenic.

About the time of Henry II. the English language was formed, the nation assuming a new character; and architecture, founded on the Norman and Saxon, yet differing from both, was invented by the English monks, whose abilities were most strenuously exercised in the work, on construction peculiar to themselves. Is it not just, therefore, to distinguish this style as English, the Gothic being applied, indiscriminately, to all edifices differing from the Grecian and Roman?

Q. Whence the early English style?

A. The introduction of shafts instead of such massive piers, and a higher mode of building, together with the pointed arch. An increased delicacy of execution and boldness of composition mark the dawn of this simple yet beautiful style, at the close of the twelfth century, reaching to the end of Edward I. 1307.

Q. What then may be advanced about the early English doors?

A. They are all pointed, at least exterior ornamented ones. There are small interior doors, with flat tops, and the sides of the tops supported by a quarter circle on each side. The large doors are mostly double, divided either by one shaft or several clustered, with a quatrefoil or other ornament over them. These doors are recessed as the Norman—bands and shafts more numerous—with hollow mouldings enriched with a peculiar ornament of this style, a *toothed projection*; but there are many doors perfectly plain. The door at Christ Church, Hants, is a fine specimen.

Q. What of the mouldings and ornaments of these doors?

A. The dripstone is clearly marked—small, and supported by a head. In many doors a trefoil and cinquefoil feathering is used. The principal moulding is an equilateral arch; but from the number of additional mouldings, the exterior becomes nearly a semicircle. Some doors have trefoil arches, shafts (round, sometimes filtered,) standing in a hollow moulding, with a variety of capitals—many plain, and many with delicate leaves running up and curling round under the cap moulding, like Ionic volutes. The bases are numerous—a plain round fillet, and the reversed ogee frequently used—mouldings cut with great boldness. York, Lincoln, and Chichester, have very fine specimens of these doors.

Q. Describe the early English windows?

A. They are narrow and lancet headed, without feathering—in some instances trefoiled. From this single shape a variety of appearance results from their combination.

Q. What is the appearance?

A. Where there are two, there is often a trefoil or quatrefoil between the heads, and divided by so small a division as to give the appearance of one large window, though in reality they are separate, having their heads from individual centres and separate dripstones. Westminster affords an example of this.

Q. What are the ornaments of these windows in large buildings?

A. Frequently they are ornamented with long and slender shafts, *banded*. There is, in all long windows of this style, one almost universal distinction, at the straight side of the window opening, by a shaft being added which is mostly insular, seldom having connection with the side, and breaking into faces, though the shafts are inserted into the sides of the doors, so as to give great variety to these openings.

Q. What is the character of the early English arch?

A. *Lancet-headed*. Composition lancet arches are used in Henry VII.'s Chapel, Westminster, and at Bath; and *flat segmental* arches in the early English part of York. The architraves of large arches, to rich buildings, are beautifully moulded like the doors, with deep hollow mouldings, *toothed*. York transept, and the nave and transept of Lincoln, are beautiful specimens.

Q. What are the distinguishing features of the early English piers?

A. There are two: 1st, the constant division of the shafts, which compose them, by one or more bands in their length; 2d, by their being ranged circular round the centre. From four to eight are set round a large circular one, as at Salisbury and Westminister. Sometimes they are so numerous as nearly to hide the shaft.

Q. Of what description are the capitals?

A. They are several, consisting of a bell with a single or double annulet under it, and a sort of copying, with more annulets above. The dividing bands are also formed of annulets and fillets, often continued under windows. The bases approach to the Grecian attic; but the reversed ogee is used.

Q. How many descriptions of early English buttresses are there?

A. Four: 1st, the old Norman, though not always so broad, and its tablets more delicate; 2d, a buttress not so broad as the flat one, but nearly the same projection as breadth, and carried up sometimes with only one set-off, and sometimes without any. These have their edges often chamferred from the window-tablet. They have a shaft in the corner, and, in large rich buildings, are occasionally panelled. 3d, a long slender buttress of narrow face and great projection, in few stages, as used in some turrets, but not very common. 4th, towards the latter part of this style, the buttress in stages was used, though not very commonly; and is distinguished by its triangular head.

Q. What may be said of the early English tablets?

A. The cornice is rich in mouldings; often with an upper slope, making the face of the parapet perpendicular to the wall below. The dripstone is diversified with several mouldings, or round, with a small hollow, occasionally ornamented with the toothed ornament or flowers. In a few buildings the dripstone is returned, and runs as a tablet along the walls—generally narrow, supported by a corbel, either a head or flower. In large buildings there are, in the ornamented parts, bands of trefoils. The tablets forming the base mouldings are sometimes of a mere slope, at others are several sets of mouldings.

Q. What of the niches?

A. The most important are found in chancels. They are various—plain, trefoil-headed, and ornamented with shafts.

Q. Describe the ornaments.

A. They will be found in regular progression, from the Norman zigzag to the delicate four-leaved flowers. Another ornament is the filling of the spaces above the choir arches with squares enclosing four-leaved flowers. In the spandrills are circles filled with trefoils, &c. Crockets were not used, and the finial was a plain bunch of three or four leaves—sometimes a knob.

Q. What is the character of the towers?

A. They rose to a much greater height than the Norman, and on them were placed the most beautiful spires. Salisbury and Chichester stand unrivalled.

DECORATED ENGLISH STYLE.

Q. Where is the marked difference in this style and the former?

A. Principally in the alterations of the windows, by throwing them into large ones, divided by mullions; introducing tracery into the heads of them, and the use of flowered ornaments, together with the alterations in the piers.

Q. How far did this style extend?

A. To the end of Edward III., 1377; perhaps 18 or 20 years longer.

Q. What is the description of the doors?

A. They are in general single; some, however, are double, often with moulded buttresses placed on each side. These doors are nearly as large as the early English double, and in ornaments much resemble them. They are almost as deeply recessed as the Norman.

Q. What of the shafts and other parts?

A. The shafts do not stand free, but are parts of the sweeps of mouldings, and instead of being cut and set up lengthwise, all the mouldings are cut on the archstone, combining strength with elegance. The capitals are formed of woven leaves in foliage; the bases consist mostly of the reversed ogee. Over the doors are canopies. The dripstone is supported by a corbel, commonly a head.

Q. What of the canopies?

A. There are three.

Q. The first?

A. The first is a common canopy of a triangular shape; the space intermediate, and the dripstone, is filled with tracery. The exterior ornaments are crockets crowned with a finial.

Q. What of the second?

A. This canopy is the ogee, running halfway up the dripstone, and then turning the contrary way, finished with a straight line running into a finial.

Q. The third?

A. This canopy is an arch running over the door, and unconnected with it, which is doubly foliated.

Q. What is to be advanced about the decorated windows?

A. They are several, although of one

principle. An arch is divided by one or more mullions, and these branch into tracery. The windows are divided into from two to nine lights.

Q. What descriptions of tracery are there?

A. Two; the first, the trefoils are all worked in the same moulding. The nave of York, the choir of Lincoln, Westminster Abbey, and Exeter, afford fine specimens. The second is the *flowing tracery*, as at York, St. Mary's, at Beverly, and other churches. In these windows large wheels are introduced; the principal moulding of the mullion has sometimes a capital and a base, and thus becomes a shaft. The architraves are not much ornamented; the dripstones and canopies are similar to those of the doors.

Q. What of the decorated English arches?

A. Those most commonly used are equilateral, but there are instances of the drop arch; the mouldings are of the last style. The dripstones are delicate mouldings, supported by heads. The arches of galleries are ornamented with foliated heads and fine canopies.

Q. What point most decidedly marks this era of architecture?

A. A new disposition of shafts being arranged diamond-wise, with straight sides, often containing as many shafts as will stand close to each other at the capital, and only a fillet or small hollow between them. The shaft which runs to support the roof often springs from a rich corbel. Exeter illustrates this beautiful style. The capital and bases are the same as described in the doors.

Q. Are there any more piers?

A. Yes; one at York Minster, where the centre shaft is larger than those on each side, and another pier, common towards the end of this style, is composed of four shafts, two-fifths engaged, and a fillet and bold hollow, half as large as the shaft, between each.

Q. Describe the decorated buttresses?

A. They are worked in stages, the setts-off being severally ornamented in moulded slopes, triangular heads, and panels. The common buttresses are frequently set diagonally, the whole differently finished.

Q. How are the tablets distinguished?

A. The cornice is very regular, with several mouldings; it principally consists of a slope above, and a deep sunk hollow, with an astragal under it. In these hollows, flowers, at regular distances, are placed; the dripstones are of the same description of mouldings, as also the tablets under the windows. The dripstones seldom run horizontal. The basement tablets are very numerous.

Q. What may be said of the niches?

A. They form one of the greatest beauties of the style, and are many, but may be divided into two grand divisions: First, the pannelled niches, the fronts of whose canopies are even with the face of the walls in which they are set; the interiors are either square, with a sloping side, or are regular semi-hexagons, and the pedestals much ornamented. The second division of niches have projecting canopies of several shapes, and are equally as ornamented.

Q. What of the ornamental parts of this style?

A. The doors, &c. already described sufficiently establish its distinguishing features from the more early style, and impress the mind with its beauties.

Q. What of the steeples?

A. They are on the principle of the former style, with more profuse decorations. The spires throughout Lincolnshire are very fine.

RESISTANCE OF FLUIDS TO BODIES PASSING THROUGH THEM.—The following notice has recently appeared of a paper entitled "An Account of a Second Series of Experiments on the Resistance of Fluids to Bodies passing through them," by James Walker, Esq. F. R. S. Civil Engineer; which was read before the Royal Society on June 6th, 1833.

The author, in a paper read to the Society in the year 1827, and printed in the Philosophical Transactions, gave an account of some experiments showing that the resistance of fluids increases in a ratio considerably higher than the square of the velocity, and that the absolute resistance is smaller than had been deduced from the experiments of the French Academy. In the present communication he states the results of his further inquiries on this subject. His experiments were made at the East India Docks, on a boat twenty-three feet long and six wide, with a stem and stern nearly vertical; one end being terminated by an angle of forty-two degrees, and the other of seventy-two degrees; and the resistance to the boat's motion being measured by a dynamometer. The results are given in tables; and it appears from them that in light vessels sharpness is more important in the bow than in the stern; but that the reverse is the case in vessels carrying heavy cargoes. From another series of experiments the author infers that the resistance to a flat surface does not exceed 1·25 lb. for each square foot, at a speed of one mile per hour; increasing for greater velocities, in a ratio considerably higher than the square of the velocity. The author concludes with some observations on the results lately obtained in Scotland, where great velocities were given to boats moving on canals, without a proportional increase of resistance.—[Proceedings of Royal Society.]

Wiswall's Yoke Cutter for dressing Spokes of Wheels.—By means of a circular saw operating in connection with the cutter wheels, the timber is squared and cut to any length that may be required, and the tenons of the spokes are then formed of any required dimensions. The spoke being presented to the action of the first cutter, or tenon wheel, by hand, the tenon is formed in less than a minute, and the body of the spoke is dressed into shape and smoothly finished, first on one side and then on the other, by two operations, in another minute, more perfectly than it could be by any mere hand tool, though used by the nicest operator. No means of forming the round tenon which is to be inserted in the rim was exhibited. This, it is obvious, must be effected by a fourth operation. The whole machine is evidently capable of a more perfect construction than that examined by the committee; but such as was exhibited in operation is evidently a useful improvement, and a labor saving machine of great profit. It saves all the time which an operator by hand necessarily expends in judging by his eye of the exactness of the shape given, and to be gained by his tool, and may be operated in artificial light, when the laborer by hand would be scarcely able to judge of his own work. There is therefore much gained by the art of making wheels, which artists in that branch of mechanics will find profitable to themselves, as they can employ their journeymen more usefully on other parts of the wheel, and in adjusting them to each other.

Blindness of Passion, or Mistakes of a Kamtschatkan Bear.—Fish, which forms

their chief nourishment, and which the bears procure for themselves in the rivers, was last year excessively scarce in Kamtschatka. A great famine consequently existed among them, and, instead of retiring to their dens, they wandered about the whole winter through, even in the streets of the town of St. Peter and St. Paul. One of them finding the outer gate of a house open, entered, and the gate accidentally closed after him. The woman of the house had just placed a large tea-machine, full of boiling water, in the court: the bear smelt to it and burned his nose; provoked at the pain, he vented all his fury upon the kettle, folded his fore-paws round it, pressed it with his whole strength against his breast to crush it, and burned himself, of course, still more and more. The horrible growl which rage and pain forced from him brought all the inhabitants of the house and neighborhood to the spot, and poor bruin was soon dispatched by shots from the window. He has, however, immortalized his memory, and become a proverb amongst the town's-people, for, when any one injures himself by his own violence, they call him "the bear with the tea-kettle."—[Capt. Kotzebue's New Voyages round the World in the Years 1823-1826.]

THE PRESS IN CHINA.—There is but one journal in the Chinese language in the whole Chinese empire; it is published at Pekin, and is called the *King Pao*, or "Messenger of the Capital." It contains all the ordinances submitted to the Emperor for approval by the six ministers of Pekin, and the various authorities of the provinces, as well as by the commandants of the military corps. The amount of subscription is a liang and an ounce of silver (about equal to twelve francs) per annum. The inhabitants of the capital alone have the advantage of receiving the paper every day at a regular hour; for, as China has no such establishment as a post-office, the country subscribers get their papers only as occasion may favor; consequently, those living at a considerable distance from the capital receive them very irregularly.

Avoylle Ferry, on Red River, Lou.,
February 8th, 1834.

To the Editor:

SIR—Inclosed is the range of the Thermometer for the month of January, regularly entered as stated. It has been the most extraordinary month ever noticed in this part of the country, for cold—cloudy—rains—and changes of weather. Most respectfully, your most obedient servant, P. G. VOORHIES.

P. S.—I wrote you particularly on 3d January, ultimo.

METEOROLOGICAL RECORD, KEPT AT AVOYLLE FERRY, RED RIVER, LOU.

For the month of January, 1834—(*Lat.* 31.10 *N.*, *Long.* 91.59 *W. nearly.*)

Date.	*Thermometer.*			*Wind.*	*Weather, Remarks, &c.*
1834.	Morn'g.	Noon.	Night.		
Jan'ry 1	51	54	53	NE—light	cloudy— { distant heavy thunder a. m.—at 4 p. m. a severe thunder shower from s to w—at 7 p. m. wind w to N—high all night
" 2	34	36	33	N—high	" —wind high all day and night
" 3	22	34	32	"	clear—night cloudy—Red River at a stand
" 4	26	26	24	N	cloudy—snow at 9 a. m. to 1 p. m., one inch deep—night clear
" 5	16	30	28	calm	clear—light clouds—night clear—severe freeze
" 6	24	38	35	"	" —snow left in the shade, where the the sun shone all gone
" 7	21	45	44	s—light	" morning—white frost—snow gone—evening and night cloudy—Red [River rising
" 8	42	52	50	NE—light	cloudy—pumpkins froze
" 9	46	50	66	SE—light	" —heavy thunder and rain—evening and night drizzling, steady
" 10	56	73	70	"	" —showers all day and night
" 11	69	76	72	s	" " " "
" 12	67	64	54	NW—high	" " a. m —evening clear—wind N, high
" 13	39	43	42	N	" " all day—and all night drizzling
" 14	38	42	39	calm	" " " " "
" 15	42	50	54	"	" " " " "
" 16	58	64	63	s—light	" " " " "
" 17	65	71	70	"	" " some sunshine " "
" 18	68	73	68	"	" " " " "
" 19	67	72	70	calm	" " " " "
" 20	66	71	72	"	" —foggy morning—evening light clouds and sunshine
" 21	50	49	41	N—high	" —light rain or drizzling all day and night
" 22	36	45	43	N—light	" " " " "
" 23	39	47	46	calm	" " " " "
" 24	42	46	45	"	" " " " "
" 25	48	62	70	"	" " evening light clouds, and wind s, light
" 26	59	53	51	NE	" "
" 27	37	33	36	"	" —rain all day, with some hail—and night hail and sleet
" 28	32	35	34	N	" —snowing light all day—and drizzling and hail all night
" 29	32	38	37	calm	" " " " " " "
" 30	38	54	54	"	" evening, sun visible through light clouds
" 31	40	44	42	w—high	" " sunshine and night clear

Red River rose this month 3 feet 5 inches—below high water, 7 feet 9 inches

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